This I attributed to the unfavourable conditions, and expected to have no trouble in seeing and measuring the small star this year. I have tried to get it this year under very favourable circumstances, when the most difficult and close pairs were comparatively easy, and have utterly failed to see the least trace of it; I am satisfied that it is now beyond the reach of any telescope in the world. The distance, according to my measures in 1890, was $4''\cdot19$. It seems hardly possible that it could have been wholly invisible this year under the conditions named, if the diminution in the distance is only $0''\cdot7$, as it should be according to the orbits of Gore and Howard. I have, therefore, concluded that the small star is approaching *Sirius* more rapidly, and that the change in distance must be decidedly larger, and certainly not less than $1''$, and consequently have drawn the shorter ellipse as more likely to represent the real motion. According to this, the distance at the time of my last attempts to see the companion was $3''\cdot1$, and it is not at all improbable that, at this distance from a star as bright as *Sirius*, it would be lost in the overpowering brilliancy of the large star, even with the Lick telescope. For these reasons I think a period of 53 years is not likely to be too short.

If this ellipse is correct the minimum distance will be $2''\cdot4$ in 1892–5, and about the end of 1894 the distance will be the same as at the time of my last measures. At that time, therefore, I trust the large telescope will supply observations which will definitely settle most of the uncertainties in the orbit of this interesting system.

Lick Observatory:
1891 *February* 26.

Invisible Double Stars. By S. W. Burnham.

In addition to a large number of double stars which have been seen and measured in the regular way, we have now a new class of binaries which may be designated as invisible double stars, since they have never been seen, and are only recognised from certain peculiarities in their appearance or motions. What is known of them is due to comparatively recent investigations. It can hardly be said that their existence has been proven in any instance, and the *primâ facie* case made is not always very strong. This might be expected when it is considered that the inference is deduced from observations more or less in error; and as the difficulty of eliminating any real change from errors of observation much larger in amount is very great, the result must be received with a good deal of allowance, and particularly so when the time embraced by the observations is limited.

The latest additions to this class of double stars are due to the spectroscope, which has shown a periodical doubling of

certain lines, which, it is inferred, must be caused by the alternate approach and recession of the components of a close double star in rapid motion. This method has both the advantages and the disadvantages of being entirely independent of former methods of observing. It is unaffected by the errors and imperfections of other observers; and the conclusions derived from it are without reference to what has been seen and done before; while on the other hand there has been as yet no confirmation of the correctness of these conclusions by the spectroscope or by the telescope. Of the revelations of this instrument in showing regular periodical changes in the spectrum there can be no doubt. Whatever there is of uncertainty is in the interpretation to be given to this phenomenon. At present the most probable, if not the only explanation is that this is due to the extraordinarily rapid revolution of two suns about each other. This may be the true theory, but it might be unwise or unsafe to claim at present that this recurrent phenomenon could not be due to some other cause. The wonders already accomplished by the spectroscope indicate its possibilities in the future, and the most advanced worker would hardly either limit its revelations hereafter, or say how much that is accepted now may not ultimately require some modification. It is always unsafe to rely too implicitly on any theory for the interpretation of observations which appears probable because no better can be suggested. Astronomy is perhaps at the head of the sciences in mistakes of this kind, although at the time these theories were brought forward, from the standpoint of knowledge as it then existed, the conclusions arrived at appeared to be sound and justifiable. Thus far the spectroscopic method of discovering new double stars does not seem to have been verified by applying it to stars known to be double, and, so far as appears, it has not been attempted. While there can be no hope of finding any known binaries with the remarkably short periods assigned to the late discoveries with the spectroscope, there must be many having sufficiently rapid relative motion to be entirely suitable to prove beyond all question that this is or is not the true explanation. A few trials of the close and short period binaries would probably settle at once whatever doubt there may be in this regard. Most of the known binaries are probably fainter than the stars discovered in this way, but that is not a very serious matter in photographing the spectrum, since the same result is reached by continuing the exposure sufficiently long. It is not necessary to go on photographing a pair in which this doubling of the lines is found. It is enough that it appears at all.

The other class of stars supposed to be double is due to apparent irregularities in their motions, as shown by meridian and micrometrical observations. With the single exception of *Sirius*, all the stars of this kind have remained visually single, or rather the supposed disturbing body has resisted all attempts to see it with the most powerful instruments. The results of the

most careful watching of these stars have been so uniformly negative that one is led to expect very little based upon evidence of this kind; and the double star discoverer would be justified in coming to the conclusion that the surest way of finding a single star is to look at one which, according to theoretical considerations, must certainly be double.

Too much reliance has been placed upon the infallibility of observations. It is always much more probable that the apparent irregularities of motion are due to the unavoidable errors of observation than that a star in the system sufficiently important to produce such a real effect should be at all times invisible.

The double star of this class which appears to stand upon the most substantial basis, and which has been most thoroughly investigated from what might be termed theoretical data, is *ζ Cancri*. From the irregularities in the motion of the more distant star around A B, it was inferred that it was a close pair, or attended by a fourth star; and Seeliger in an exhaustive analytical investigation has shown that the third star, C, with its invisible companion appear to rotate in about $17\frac{1}{2}$ years. Perhaps no double star in the heavens has been more systematically measured than *ζ Cancri*. For more than 60 years we have more or fewer measures every year. During this interval the known close pair, A and B, have completed more than one revolution; and around these stars C has moved through an arc of 35°. This triple received special attention at Pulkowa, and the observations of O Σ cover a period of 40 years. Dembowski also measured it for many years, and most of the leading double star observers have given it more or less time. Therefore, the material to work with for any purpose is as good as would be found relating to any pair. The peculiar grouping of the positions of C was noticed a long time ago in the Pulkowa measures, and the existence of some third disturbing body suspected. Then followed finally Seeliger's investigation previously referred to.

In examining a matter of this kind the first step is to lay down all the observations accurately to scale, so that the eye can take in at a glance the entire movement of the stars as shown by the measures. This cannot be done by an inspection of tabulated results in whatever shape they may be put. The second step is to ascertain whether the peculiarities of motion are found in other and similar pairs, because, if these apparently discordant positions are rather the rule than the exception, it would go far to prove that they were due to inaccurate measures, and not to any mysterious disturbing influence.

An examination of the measures of the star C of *ζ Cancri* shows that the positions of that star are not distributed uniformly, but grouped more or less regularly along the path of its motion. This has been taken to mean that from some disturbing cause, not heretofore recognised, the motion of the star is alternately accelerated and retarded; and as this could be produced in one way

by a fourth, unseen, star, it is therefore assumed that C is attended by a dark star, the mass of which is about the same as that of C.

Being curious to know just how far such an assumption would appear to be warranted by the actual observations, I have selected measures representing as nearly as possible every year since the first observation by Struve, and have laid them down on a large scale, shown in a reduced form in the accompanying diagram (plate 6). All of the years from 1826 to 1888 inclusive are represented except 1827, 1829, 1830, 1834, 1837, 1838, 1839, and 1841. Preference has been given to the measures of Struve and O. Struve, and, when these were not to be had, to the measures of Dembowski, Dunér, Dawes, and other experienced observers. The vertical scale on the left gives the position-angles, and the scale on the top the distances. The sequence of the measures is shown by the lines joining the successive places. The time-scale on the right is made by dividing the whole interval covered by the measures into equal parts.

It is unnecessary to refer to what is shown by this diagram. The whole history of this star, so far as the measures are concerned, can be seen at a glance. The arrangement of the observed positions is certainly suggestive of some variation; and there is no doubt that the three groups of positions occur at substantially equal intervals. The question whether this is due to errors of observation is not an easy one to solve, if we consider only these observations. To show the relation between the observed and theoretical positions of C, I have given the latter, as computed by Seeliger, by the side of the actual measures, and on the same scale.

The next step was to find out, if possible, if any such peculiar motion of the companion star was shown by the measures of any other pair. To make the case parallel, it was necessary to select pairs having slow motion in position-angle of 15° to 35°, and which had been frequently measured during the same or an equal period of time. There are stars of this kind which have about the same angular velocity as ζ *Cancri*, differing not enough in distance to sensibly change the errors of measurement, and which have been measured largely by the same observers who observed the pair in question. Of course these would all be old and familiar pairs, and necessarily among the easiest objects in the heavens to measure. Among the stars selected as suitable for this purpose are *Castor*, ϵ^2 *Lyræ*, ζ *Aquarii*, γ *Leonis*, ι *Leonis*, ξ *Scorpii*, and ϵ *Hydræ*. I have plotted the measures of each of these stars in the same way (plates 7 to 9), and on the same scale, as the measures of ζ *Cancri*, and they are therefore strictly comparable. It should be noted that none of these stars have been as frequently measured as ζ *Cancri*, and, therefore, any peculiarity in the arrangement of the positions would be less prominently marked.

We find in each of these stars not only the same tendency to a grouping of the positions, but in some of them they occur at

intervals quite as regular as those of ζ *Cancri*. Take *Castor*, for instance (for which we have fifty-one measures, 1819–1878), and we find, if these groups of measures are to be taken as real, that the companion may be supposed to be revolving round a dark star once in about eight years. The circles which I have drawn round the groups are 0″·5 in diameter, and the years attached to them give the mean dates of the included observations.

In ζ *Aquarii* (1821–1871) we have similar discordances, and with a little imagination these variations might appear to recur periodically.

In ϵ^2 *Lyræ* (1819–1877) there is the same appearance, more or less regular, of accelerated and retarded motion, but only half of the years are represented by measures, and, therefore, the grouping of the measures is not so well shown.

In ξ *Scorpii* there is one very strongly marked group, with few and scattering positions on each side. This is a triple star, similar in all respects to ζ *Cancri*, the third star in each case being measured from a close pair. About three-fourths of the years (1820–1877) are represented by measures.

In γ *Leonis* three distinct groups are fairly well marked at intervals of ten years, ending with 1872. The years for which there are no measures are all previous to 1845, with the single exception of 1849. The angular motion is less than that of most of the other pairs represented.

In ε *Hydræ* (1825–1889) we have three fairly well defined groups with an apparent period of thirteen years. There are twenty-one years not represented by measures, of which fifteen are previous to 1851. Eight of these missing years would come within the range of the 1844 period. It is well known that in this instance the principal star is a close pair, but it is much too close to have any influence whatever on the measures. The maximum distance probably does not exceed 0″·25, and this, with unequal components, could not possibly affect the measures with any telescope. Even with the 36-inch it would not be recognised as a double star with any power that would ordinarily be used in measuring C. There is probably no doubt that Schiaparelli's pair is moving, and perhaps rapidly. My measures indicate a motion in angle of about 16° per annum.

It is evident that orbits could be computed for at least some of these stars which would satisfy the observations as well as has been done in the case of ζ *Cancri*, since it is only necessary to have the groups of measured positions occur at substantially regular intervals.

It should be stated that these apparent peculiarities of motion are not exceptional in stars of this class. I have given here the diagram of every star whose record I have investigated, with the single exception of ε *Equulei*, which is omitted because the whole angular change is only 10°, and this is but a little more than the average error of many of the measures. I am satisfied from these examples, and from looking over the measures of

other stars of this kind, that similar discordances or peculiarities will be found in all. A partial explanation at least will readily suggest itself when it is remembered that all of these stars have a general movement of only about half a degree per year; and as the average error of the measures of angle is decidedly more than that, and in many instances several times that amount, it follows that apparent retrograde movements and groups of positions must be shown where there are annual measures. There is nothing extraordinary in such a case of the positions of even half a dozen years falling in about the same place; and that there is nothing wonderful in the crowding together of the positions at nearly equal intervals is demonstrated by the frequency with which it occurs.

It is unnecessary to discuss the question how far, if at all, ζ *Cancri* differs from the other pairs referred to. With the graphical representations of the several movements before him, each one can decide this matter for himself as intelligently as if he had canvassed and investigated the whole subject. It is not so much a matter for mathematical inquiry as for the exercise of sound judgment in considering whether any or all of these discordances are due to errors of observation.

The observer who has spent a good many years of his life in the discovery of new double stars, and particularly after finding new and more difficult components in scores of the old pairs, would be the last to say that a component of ζ *Cancri*, or of any other double star in the heavens, was not itself double. One never looks at an old pair without expecting to find a new star, and it is certainly as promising a place for discovery as any other. The most that any observer can say is that any given star, if double, is beyond the power of his telescope.

Leaving out of view for the moment the various stars mentioned as illustrations of apparent changes, and assuming that the variations of the star C of ζ *Cancri* are real, there is another, and perhaps a more natural explanation of variable motion, and that is that the centre of gravity of A and B is not in the middle of the line joining the two stars. It is infinitely more probable that two stars of about the same brightness have unequal masses than that a third nearly as bright star is balanced by one of the same mass which cannot be seen at all. Of course this would not produce a similar recurrence in point of time, but would help to explain the variations shown by the measures.

There is also a special possible source of error in the measures of C, due to the fact that in many of the observations A and B are treated as one star, and the measures made from the assumed middle of the double or elongated disc. Every practical worker with the micrometer knows how much uncertainty in both angles and distances there is in measures made under such circumstances. This is not a close pair, and even when the distance is minimum it would be called a comparatively easy object with

modern telescopes; and yet these stars at this time are more than 10° apart as seen from C. This would require the observer to guess at the middle point of an arc of 10°, which is constantly changing its angle with the bisecting line. That this alone would be the cause of considerable error there can be no doubt. It is not easy to bisect a line at right angles; it is still more difficult to bisect an oblique line. It may be worth while to point out as a fact, having possibly some significance in this connection, the relation of these lines at the times of the critical positions of C. The dates given are the mean epochs of the groups of C as actually measured.

	A B and C.	A and B.	
	°	°	°
1842	151	360	29
1859	141	290	31
1877	131	100	31

It will be seen that the line joining A B and C makes practically the same angle with A and B at each of these periods, or about 30°.

Another fact may be mentioned as a curious coincidence, if nothing more. The orbital motion of A B is in the same direction as the general motion of C around that pair. In 17 years C moves forward 10°, so that A B make exactly a quarter of a revolution with reference to the place of C during one rotation of the theoretical pair, and therefore the lines connecting the three stars have the same angular rotation for each period, and recur in the same order.

If the apparent change in C is not due to any fourth dark star, but is caused by errors of observation, augmented perhaps by some of the sources of error mentioned, it then follows that with a repetition of the same kind of observations we shall have the same grouping of positions at substantially the same intervals; and, therefore, such a recurrence will be no proof of the existence of a fourth star. In view of the actual measures of this star, taken in connection with the similar variations shown in the measures of other stars where there is no question of any disturbing influence, it seems certain that special observations will have to be made of this star, using every precaution to eliminate as far as possible all sources of error in order to determine, first, whether there is any real variation in its motion; and secondly, if there is, to what is it due? One method is to measure it from A and B separately, with instruments of sufficient separating power so that the other member of the closer pair shall affect the bisection of the one being measured from as little as possible. If measurements of this kind are not made with instruments of the best definition, and under the most favourable atmospheric conditions, it is quite certain that there will be an appreciable error in the distances, and particularly in the angles; and even then it may not be possible to get rid of

the effect of one star in measuring another star so near it. Obviously the best way, and the only independent one, would be to measure C from some entirely outside point.

The nearest star at all available for this purpose is D. M. $+18°$, 1870, a 9·1 magnitude star, which is 286″ from C in the direction of 108°·2. As this is nearly in the line joining AB and C (123°) it would be sufficient to measure the angle only, and this could be done with great accuracy; but the distance is so great, the resulting errors of observation might still be too large to determine whether the angular motion of C is uniform. However, some of the other pairs mentioned doubtless have convenient stars to be tested with the micrometer in this manner, for it can hardly be questioned that the same explanation will apply equally to all.

Perhaps the best independent method in the case of *ζ Cancri* would be to compare C with a neighbouring star, measuring annually the difference of declination with the meridian-circle. The changes in the place of C, according to theory, should be now, and for several years to come, principally in declination, and this should be shown by differential meridian observations covering a series of years. There is a 7·4 magnitude star 2^m following and about 1′·5 north which would be suitable for this comparison. The position-micrometer could be used with this star to measure the difference in declination.

Lick Observatory:
1891 *March* 24.

A Comparison of the North Polar Distances of the Nautical Almanac for 1880 *with the Cape Catalogue, the Greenwich Ten-year Catalogue, and Boss's Standard Star Places for* 1880. By W. Grasett Thackeray.

The following comparison was made for the purpose of seeing whether the Greenwich Ten-year Catalogue Star Places for 1880 would corroborate the results of a comparison between the *Nautical Almanac* and Cape General Catalogue for 1880, given by Mr. Stone in a paper published in the *Monthly Notices*, xl. 2, pp. 57–70.

In this paper it was shown that the discordances in N.P.D. (Cape–Greenwich), arranged for every six hours of R.A., gave the following corrections after applying the general mean correction of $-0''{\cdot}31$:

0^h–6^h.	6^h–12^h.	12^h–18^h.	18^h–24^h.
$-0''{\cdot}24$	$-0''{\cdot}11$	$+0''{\cdot}39$	$+0''{\cdot}06$

and it was further remarked that the observations corresponding to the result grouped under 0^h–6^h were made during the dry